them, by attracting in like manner also the attention of insects, which, visiting the flowers for their own profit, at the same time unconsciously bring to the plant the great advantage of cross-fertilisation. Hence we understand that bright-flowered varieties, whenever produced by any cause, might be preserved by natural selection, and at last remain the only survivors among all the concurrents of the same species. Thus, the occasional appearance of gaily-coloured varieties granted as a matter of fact, and the peculiarities of colour supposed to be hereditable, we are enabled by Darwin's theory to explain the variety of colours met with in flowers. But we should always bear in mind that we are at present quite ignorant of the chemical processes by which certain colours are produced in the flowers, and of the physical or organic causes by which these chemical processes were effected when they first appeared and are effected in every subsequent generation. Reflecting on the first origin of the adaptation of flowers to the cross-fertilisation by insects, and considering that the oldest and most primitive phanerogamous plants which still exist, the Gymnospermæ, are exclusively fertilised by the wind (are *anemophilous*), whilst the enormous majority of Angiospermæ is provided with flowers adapted to cross-fertilisation by insects (*entomophilous*), we cannot doubt that the original manner of fertilisation of phanerogamous plants was fertilisation by the wind, and that the first plants which adapted their flowers to cross-fertilisation by insects were anemophilous ones, either Gymnospermæ or the next descendants of them. Nevertheless the flowers of many Gymnospermæ (Abietinæ) present a beautiful colour, which attains its culmination during the disseminating of the pollen.[*] This beautiful colour is apparently neither of any use to these plants, which are regularly cross-fertilised by the wind, nor can have been inherited from ancestors to which it was useful. We may therefore also in this case, without hesitation, regard the colour as a merely accidental phenomenon, which, secondarily produced by the more active chemical processes during the time of flowering, disappears again in the same degree as the intensity of development decreases in the cones. Probably the gaily-coloured perianths of the entomophilous Angiospermæ have originated in a similar manner.

Independently of possible physical effects, natural selection is evidently without any influence as to colours, unless animals are attracted or repelled by them. Consequently not only the first origin of bright-coloured flowers, but also the change of colour in the flowers after the ovaries are set, is altogether foreign to the effects of natural selection. It is as indifferent to an entomophilous plant whether its flowers, after having been fertilised, grow paler or darker, as it is to an anemophilous plant whether its flowers are attractive to insects or not. In most cases, indeed, flowers change while fading into paler and less conspicuous colours, but often also their colour remains unaltered or even grows more conspicuous. Old flowers of *Melampyrum pratense*, for instance, which, not having been cross-fertilised by insects, regularly fertilise themselves, are always reddish-yellow, whilst younger ones are yellow.

As to *Fumaria capreolata*, alluded to in Mr. Moggridge's letter (NATURE, vol. ix. p. 423) I have never had the opportunity of observing its flowers myself, but from Hildebrand's account ("Jahrb. f. wissensch. Bot." vii. p. 452) I believe that it is restricted to regular self-fertilisation, cross-fertilisation by insects not, indeed, being impossible, but taking place very exceptionally; for it has lost, probably from permanent disuse, the elasticity of the cap formed by the inner petals, which in other fumitories secures cross-fertilisation in case of the repeated visits of insects. If this presumption of mine be right, it would the more explain Mr. Moggridge's observation; for in this case the colour of the flowers of this fumitory, inherited from ancestors to which it was quite useful, would be almost useless to this degenerated descendant, and therefore almost withdrawn from the influence of natural selection.

HERMANN MÜLLER

Lippstadt, April 4

Conference for Maritime Meteorology

SOME of your readers may have noticed in the Report of the Proceedings of the Meteorological Congress at Vienna that it was decided to be advisable to convene a fresh Conference for maritime meteorology, in order to reconsider the decisions of the Brussels Conference in 1853.

The matter was handed over to the Permanent Committee, and by them delegated to a sub-committee composed of the following members:—

Prof. Buys Ballot (Holland)
Prof. Mohn (Norway)
Capt. E. Mouchez (France)
Dr. G. Neumayer (Germany)

with myself.

The sub-committee have nearly decided on a form of programme for the proceedings, and there are hopes that the Conference will meet in London in the month of August or so. Endeavours will probably be made to induce H.M.'s Government to issue the invitations, and thereby to give an official character to the Conference.

ROBERT H. SCOTT

Herbert Spencer and *à priori* Truths

ABSENCE from town has delayed what further remarks I have to make respecting the disputed origin of physical axioms.

The particular physical axiom in connection with which the general question was raised, was the Second Law of Motion. It stands in the *Principia* as follows:—

"*The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.*

"If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively. And this motion (being always directed the same way with the generating force), if the body moved before, is added to or subducted from the former motion, according as they directly conspire with or are directly contrary to each other; or obliquely joined, when they are oblique, so as to produce a new motion compounded from the determination of both."

As this, like each of the other laws of motion, is called an axiom;[*] as the paragraph appended to it is simply an amplification, or re-statement in a more concrete form; as there are no facts named as bases of induction, nor any justifying experiment; and as Newton proceeds forthwith to draw deductions, it was a legitimate inference that he regarded this truth as *à priori*. My statement to this effect was based on the contents of the *Principia* itself; and I think I was warranted in assuming that the nature of the laws of motion, as conceived by Newton, was to be thence inferred.

The passages quoted by the *British Quarterly Reviewer* from Newton's correspondence, which were unknown to me, show that this was not Newton's conception of them. Thus far, then, my opponent has the best of the argument. Several qualifying considerations have to be set down, however.

(1) Clearly, the statements contained in the *Principia* do not convey Newton's conception; otherwise there would have been no need for his explanations. The passages quoted prove that he wished to exclude these cardinal truths from the class of hypotheses, which he said he did not make; and to do this he had to define them.

(2) By calling them axioms, and by yet describing them as principles "*deduced* from phenomena," he makes it manifest that he gives the word axiom a sense widely unlike the sense in which it is usually accepted.

(3) Further, the quotations fail to warrant the statement that the laws of motion are proved true by the truth of the *Principia*. For if the fulfilment of astronomical predictions made in pursuance of the *Principia* is held to be the evidence "on which they chiefly rest to this day," then, until thus justified, they are unquestionably hypotheses. Yet Newton says they are not hypotheses.

Newton's view may be found without seeking for it in his letters: it is contained in the *Principia* itself. The scholium to Corollary VI. begins thus:—

"Hitherto I have laid down such principles as have been received by mathematicians, and are *confirmed* by abundance of experiments. By the two first Laws and the two first Corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola; experience *agreeing* with both," &c.

Now as this passage precedes the deductions constituting the *Principia*, it shows conclusively, in the first place, that Newton did not think "the whole of the *Principia* was the proof" of the

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[*] See Strassburger's memoir in "Yenaische Zeitschrift," vi. band, 2 heft, pp. 249-261.

[*] It is true that in Newton's time, "axiom" had not the same rigorously defined meaning as now; but it suffices for my argument that, standing unproved as a basis for physical deductions, it bears just the same relation to them that a mathematical axiom does to mathematical deductions.

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laws of motion, though the Reviewer asserts that it is. Further, by the words I have italicised, Newton implicitly describes Galileo as having asserted these laws of motion, if not as gratuitous hypotheses (which he says they are not), then as *à priori* intuitions. For a proposition which is *confirmed* by experiment, and which is said to *agree* with experience, must have been entertained before the alleged verifications could be reached. And as before he made his experiments on falling bodies and projectiles, Galileo had no facts serving as an inductive basis for the Second Law of Motion, the law could not have been arrived at by induction.

Let me end what I have to say on this vexed question by adding a further reason to those I have already given, for saying that physical axioms cannot be established experimentally. The belief in their experimental establishment rests on the tacit assumption that experiments can be made, and conclusions drawn from them, without any truths being postulated. It is forgotten that there is a foundation of preconceptions without which the perceptions and inferences of the physicist cannot stand—*preconceptions which are the products of simpler experiences than those yielded by consciously-made experiments.* Passing over the many which do not immediately concern us, I will name only that which does,—the exact quantitative relation between cause and effect. It is taken by the chemist as a truth needing no proof, that if two volumes of hydrogen unite with one volume of oxygen to form a certain quantity of water, four volumes of hydrogen uniting with two volumes of oxygen will form double the quantity of water. If a cubic foot of ice at 32° is liquefied by a specified quantity of heat, it is taken to be unquestionable that three times the quantity of heat will liquefy three cubic feet. And similarly with mechanical forces, the unhesitating assumption is that if one unit of force acting in a given direction produces a certain result, two units will produce twice the result. Every process of measurement in a physical experiment takes this for granted; as we see in one of the simplest of them—the process of weighing. If a measured quantity of metal, gravitating towards the earth, counterbalances a quantity of some other substance, the truth postulated in every act of weighing is, that any multiple of such weight will counterbalance an equi-multiple of such substance. That is to say, each unit of force is assumed to work its equivalent of effect in the direction in which it acts. Now this is nothing else than the assumption which the Second Law of Motion expresses in respect to effects of another kind. "If any force generates a motion, a double force will generate a double motion," &c., &c.; and when carried on to the composition of motions, the law is, similarly, the assertion that any other force, acting in any other direction, will similarly produce in that direction a proportionate motion. So that the law simply asserts the exact equivalence of causes and effects of this particular class, while all physical experiments *assume* this exact equivalence among causes and effects of all classes. Hence, the proposal to prove the laws of motion experimentally, is the proposal to make a wider assumption for the purpose of justifying one of the narrower assumptions included in it.

Reduced to its briefest form the argument is this:—If definite quantitative relations between causes and effects be assumed *à priori*, then, the Second Law of Motion is an immediate corollary. If there are not definite quantitative relations between causes and effects, all the conclusions drawn from physical experiments are invalid. And further, in the absence of this *à priori* assumption of equivalence, the quantified conclusion from any experiment may be denied, and any other quantification of the conclusion asserted.

HERBERT SPENCER

MR. SPENCER'S letter in NATURE, vol. ix. p. 420, is likely to give to such of your readers as have not followed the controversy in which he is engaged a false notion of the issues therein. Mr. Spencer writes as though the views of the nature of physical truth that were objected to by Prof. Tait and myself amounted to the ascription of our knowledge of sundry physical laws to organised ancestral instead of individual experiences. In one portion of his reply to me he intimates the same, as, for instance, where he says of me:—

"His argument proceeds throughout on the assumption that I understand *à priori* truths after the ancient manner as truths independent of experience; and he shows this more than tacitly where he 'trusts' that he is attacking one of the last attempts to deduce the laws of nature from our inner consciousness. Manifestly a leading thesis of one of the works he professes to review is entirely unknown to him—the thesis that forms of thought, and consequently those intuitions which those forms of thought involve, result entirely from the effects of experiences organised and inherited" ("Replies to Criticisms," p. 332).

But, in his "First Principles," Mr. Spencer expresses himself far too clearly for him to be able to assign the above as his views at that time on these so-called *à priori* truths. Speaking of the indestructibility of matter, one of the three truths in question, he says:—

"The annihilation of matter is unthinkable for the same reason that the creation of matter is unthinkable—and its indestructibility thus becomes an *à priori* cognition of the highest order—*not one that results from a long continued registry of experiences gradually organised into an irreversible mode of thought;* but one that is given in the form of all experiences whatever."

For the second of the truths he claims a similar authority; while for the third—the Persistence of Force—he claims a yet higher warrant:—

"Deeper than demonstration—deeper even than definite cognition—deep as the very nature of mind is the postulate at which we have arrived (*i.e.* the Persistence of Force). *Its authority transcends all other whatever; for not only is it given in the constitution of our own consciousness,* but it is impossible to imagine a consciousness so constituted as not to give it." ("First Principles," p. 192).

Had Mr. Spencer confined himself to defending such an *à priori* origin of physical truths as he now seems inclined to put forward, I should never have compared his theories to those of the Ptolemaists. But I can leave it with confidence to the readers of NATURE to decide between us as to whether the above passages do not show that at the time when they were written Mr. Spencer understood *à priori*, as there applied, in a manner very like the "ancient manner," and whether he did not maintain that these *à priori* truths were indeed "truths independent of experience."

THE AUTHOR OF THE ARTICLE ON HERBERT SPENCER
IN THE BRITISH QUARTERLY REVIEW

[The Editor, very properly wishing, I doubt not, to end the controversy, has sent to me the foregoing letter in proof. My comment on it is very brief.

Had the reviewer read the "Principles of Psychology," placed at the head of his article apparently for form's sake only, he would, not, I think, have made the above rejoinder.

That view of the *à priori* origin of physical truths which the Reviewer now seems to think defensible is the view implied in "First Principles" and the view set forth in the "Principles of Psychology," published years before. Tacitly throughout that work, and explicitly near the end, in a chapter on "Reason," the doctrine is that the "forms of thought" themselves are the products of experience. If the nervous system as a whole and in all its structures has been evolved by converse between the organism and the environment, the fundamental principles of its action, the very "forms of all experiences" have been evolved. Experience itself grew into definiteness gradually. And if the very form of our thought, the very frame-work of our consciousness, has been thus moulded, the inability to conceive a mode of thinking fundamentally different, is simply the result of inability to invert the fundamental action of the structures by which we think.—H. S.]

On the Word "Axiom"

IN reference to the controversy between Mr. Spencer and his reviewer about Sir I. Newton's calling his laws of motion "axioms," it is to be observed that there is a certain ambiguity in the word. "Axiom" is from ἀξιόω (I demand), and would thus signify a first principle to be taken for granted. It does not, of course, carry with it the meaning of a necessary judgment which cannot be contradicted. Whatever may be considered the ground of Euclid's "axioms" so called, Euclid himself did not apply that name to them; but the first nine he called "common notions," and the last three (which are peculiar to geometry) he placed among the postulates (ὁμολογήματα), and heads them with "let it be granted." Now it is clear, from Newton's own words, that in calling his Leges motûs "axioms," he does not imply that they are *necessary* judgments, but that he requires them first of all to be granted (however established) in order to the following reasoning. In other words, they are postulates, like Euclid's last three "axioms." In our modern use of the words "axiom," "axiomatic," there is always implied the *ground* why a proposition is demanded as granted, viz., because its necessity is self evident; but this wider use is not required by etymology, or (I think) in interpreting all ancient writings.

F.M.S.

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